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Do Entry Regulations Deter Entrepreneurship and Job Creation? Evidence from Recent Reforms in Portugal

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ABSTRACT

Recent research has suggested that the reduction of entry regulation can promote firm entry and job creation, but little is known about the quality of firms and jobs created through these reforms. To shed light on this question, we employ data from Portugal, a country which implemented one of the most dramatic and thorough policies of entry deregulation in the industrialized world. The impact of these major changes can be traced with a matched employer-employee database that provides unusually rich information on the quality of founders and employees associated with the new firms. Our assessment indicates that the short term consequences of the reform were just as one would predict with a standard economic model of entrepreneurship: The reform resulted in increased firm formation and employment, but mostly among "marginal firms" that would have been most readily deterred by existing heavy entry regulations. These marginal firms were typically small, owned by relatively poorly-educated entrepreneurs, operating in the low-tech sector (agriculture, construction, and retail trade). These firms were also less likely to survive their first two years than comparable firms that entered prior to the reform. The social impact of entry deregulation may be limited by the quality of the firms it creates.

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1 Introduction

Do burdensome entry requirements hamper the creation of new firms and stand in the way of economic growth? There are solid theoretical reasons to believe that government-imposed fixed costs to firm formation deter entrepreneurship and reduce labor demand. Two important strands of empirical work suggest that existing barriers have economically important effects in practice.

One set of analyses is based on cross-country comparisons. These papers compare countries in terms of the stringency of entry regulation, and ask if that variation is correlated with measures of economic performance. For instance, in an influential paper, Djankov, La Porta, Lopez-De-Silanes and Shleifer (2002) establish that the official costs of business entry are extremely high in many countries, and they show that onerous entry regulation is associated with higher corruption and a higher concentration of activity in the informal sector. They find no evidence to suggest that high entry costs create value through improved quality of either publicly or privately provided goods. Many subsequent papers have used similar empirical strategies to evaluate the impact of the regulation of business entry, suggesting that such regulation is associated with reduced job creation (Ciccone and Papaioannou, 2007), higher industry concentration (Fisman and Sarria-Allende, 2004; Klapper et al., 2006), and reduced entry of new firms (Klapper et al., 2006; Dreher and Gassebner, 2007; Ciccone and Papaioannou, 2007; Bjørnskov and Foss, 2008)¹.

Of course, empirical work that relies on cross-national correlations leaves researchers with a difficult inference problem—an issue that receives an extended and insightful discussion in the original work of Djankov et al. (2002). While it is important to know that heavy entry regulation is associated with poor economic performance within countries, that correlation alone is not sufficient to assess the likely consequence of regulation reform in practice. After all, as Djankov et al. (2002) demonstrate, countries with heavy entry regulation also are less likely to have “good government” along a number of dimensions. One might be concerned, for instance, that entry regulation reform, on its own, would have limited success in a country with other deep structural economic and political problems.

As an alternative to cross-country analyses, a second complementary literature seeks to directly assess the consequences of policies that reduce firm entry costs using time-, region- and/or industry-specific variation in entry costs created by policy changes within particular countries. Bertrand and Kramarz (2002), for example, carefully evaluate entry barriers in France’s retail industry. That paper presents evidence that stronger entry deterrence increased industry concentration and reduced employment growth. A second prominent example is the work by Aghion et al. (2008), which shows that the dismantling of the License Raj in India (a system of central controls that regulated entry and output expansion) resulted in industry growth, but did so in ways that were tied to other features of the states in which

¹More generally, to date nearly 200 academic articles have utilized either the original Djankov et al. (2002) data set or updated versions published by the World Bank, as part of the World Bank’s *Doing Business Project*, which tracks regulatory reforms in 181 countries. The World Bank indicators are widely used. For example, the Millennium Challenge Corporation, set up by the United States to channel aid to developing countries, uses these and similar indicators to screen applicants for U.S. development aid. Djankov (2009) provides more examples and details.

the reforms were occurring.²

Our paper contributes to this second stream of research by evaluating the impact of entry deregulation in Portugal. Prior to 2005 the barriers facing entrepreneurs in Portugal were among the highest in Western Europe, according to World Bank measures. In 2005 Portugal implemented the “On the Spot Firm” program (*Empresa na Hora*) which established “one-stop shops” that offered prospective entrepreneurs significantly reduced administrative fees, and simplified incorporation procedures. The reform reduced the time delay of legal incorporation from several months to as little as one hour, and also reduced monetary fees that initially were on the order of 2000 Euro to less than 400 Euro. Over the next several years, these offices were opened across the country. As a consequence of this reform, Portugal’s ranking in the World Bank’s “Doing Business Index” moved from 113rd out of 155 countries to 33rd, and Portugal was cited by the World Bank as the top reformer in business entry regulation in 2005/2006. Portugal thus provides an excellent context in which to evaluate the impact of substantial entry regulation reform. What effect did this reform have on firm and job creation? What types of entrepreneurs benefited from this reform? How well did any new “marginal” firm entrants perform over time?

To answer our key questions, we use micro-level data and examine the implementation of the Portuguese business registration reform in different counties at different time periods. Our data provide detailed information on the new firms established in each county between 2000 and 2008. For each firm, we were able to gather information on size, founder characteristics, and firm survival. The fact that adoption of the reform varied across county and time is helpful for our identification strategy. The richness of our data allows us to compare the characteristics of new firms created by the reform to those that emerged prior to its implementation, providing a dimension of analysis that has been missing in the previous literature.

We find that in the short run, the Portuguese reform increased the number of business start-ups by approximately 17 percent, and created approximately seven new jobs per 100,000 county inhabitants per month in eligible industries. There is evidence to suggest that the “marginal firms”—firms that entered as a consequence of reduced entry costs—were operated by proprietors who were disproportionately older, more female, and less educated than proprietors of infra-marginal firms. The marginal firms were typically low-tech, e.g., in the agricultural, retail trade, and construction industries. We also find that start-ups established after the program are smaller and less likely to survive in the first two years than firms founded in the absence of the program. Taken together, these results suggest that entry deregulation has had a modest positive impact on firm and job creation in Portugal. The barriers that existed prior to deregulation were a significant impediment for some entrepreneurs, but less of an impediment to the larger, high-quality firms that are most likely to create substantial economic growth. These lessons are likely to extend beyond the Portuguese context of this study, and suggest that the ultimate impact of business entry deregulation may fall somewhat short of current expectations.

²See also Chari (2007) on the License Raj. Other relevant examples include analyses of Russian reform (Yakovlev and Zhuravskaya, 2007) and entry regulation reform in Mexico (Kaplan et al., 2009; Bruhn, 2008) and Brazil (Monteiro and Assunção, 2006).

The remainder of this paper is structured as follows. In section 2, we provide a brief overview of the literature on entry regulation. Section 3 develops a simple model for the purpose of highlighting expected the effects of entry deregulation on entrepreneurial outcomes. Section 4 describes the Portuguese business simplification reform in further detail. A description of the data follows in section 5. In section 6, we present the empirical strategy, results and robustness checks. Section 7 concludes.

2 Entry Regulation: Theory and Existing Evidence

Two contrasting theoretical views of entry regulation have long been debated: “public choice” and “public interest” theories. The first regards regulation as socially inefficient, and as a mechanism to create and extract rents. This view holds that regulation either benefits bureaucrats and politicians by collecting bribes from entrants (De Soto, 1989) or benefits incumbent firms by deterring the competition of potential entrants (Posner, 1975; Peltzman, 1976; Stigler, 1971). In contrast, public interest theory provides a potential rationale for entry barriers: regulation might help correct market failures and achieve socially superior outcomes (Pigou, 1938), for example, screen new firms, thereby reducing low-quality products or damaging externalities.

Recent empirical evidence does not appear to square with public interest theory. Many papers, some of which we have cited above, suggest that entry regulation indeed has the expected first-order effect of reducing firm entry and affecting the market structure.³ Djankov (2008), among others, argue that the main beneficiaries of complex entry regulation are often incumbent firms, which see their rents and competitive position protected by burdensome entry requirements. Considerable effort has also been devoted to studying the link between regulation and macro-economic outcomes such as employment, productivity and growth (Bertrand and Kramarz, 2002; Alesina et al., 2005; Djankov et al., 2006; Yakovlev and Zhuravskaya, 2007; Chari, 2007; Kaplan et al., 2009; Aghion et al., 2008; Barseghyan, 2008; Bruhn, 2008). Stricter regulation is linked to slower growth, lessened productivity, inhibited investment and decreased employment (particularly in high-skilled jobs, according to Barseghyan (2008)). Some evidence suggests that entry regulation induces business to operate in the informal sector (Djankov et al., 2002; Monteiro and Assunção, 2006).

In an effort to take seriously the concerns raised by public interest theory, some work focuses on the impact of regulation on market failures such as sub-standard product quality, pollution or public health (Djankov et al., 2002; Yakovlev and Zhuravskaya, 2007; Bruhn, 2008). Most work along these lines finds no evidence of beneficial effects of entry regulation.

As we have mentioned, much of the existing work relies on cross-national variation. Causal inference here is difficult. For example, governments might regulate more heavily in countries where there are more market failures, or regulators might focus effort on rent extraction in countries where such extraction is made possible by other structural deficits in governance. Omitted variables can jointly drive economic outcomes and regulation. One

³See, e.g., Djankov et al. (2002); Bertrand and Kramarz (2002); Fisman and Sarria-Allende (2004); Klapper et al. (2006); Ciccone and Papaioannou (2007).

potential path is to search for instruments that drive regulation decisions,⁴ or to draw lessons from such specific quasi-experiments as German reunification (Prantl and Spitz-Oener, 2009) or reform in Russia (Yakovlev and Zhuravskaya, 2007).

While useful, these approaches are not a substitute for empirical evaluations of actual policy shifts that change entry regulation. There are two reasons why our examination of the Portuguese policy shift holds particular promise. First, the policy shift was quite simple and, as we discuss below, dramatic. Second, we have access to extraordinary panel data on firms, their employees, and their founders. Thus, in comparison to the extant literature, we are able to evaluate the impact on firm entry and job growth, and also the *kinds* of start-ups and entrepreneurs that appeared to benefit from deregulation. Previous empirical work has suggested that entry regulation results in increased firm size (Desai et al., 2003; Fisman and Sarria-Allende, 2004; Klapper et al., 2006). Beyond that, little is known about the nature and quality of the firms that can be expected to enter when governments adopt deregulation. This is a significant gap in the literature because the social impact of deregulation will be a function not only of the number of new firms brought into the market, but also their quality, size, performance, and longevity.

3 A Simple Model

To fix ideas, and set the stage for the empirical analysis that follows, we develop a simple model here that draws on the logic given in the seminal work of Lucas (1978).

We evaluate an economy in which agents can choose between three options in each period: (i) home production (or leisure), which provides monetized utility w , (ii) supplying one unit work in a competitive labor market, or (iii) becoming an entrepreneur and hiring other agents. In equilibrium, agents are indifferent between the first two possibilities, and we assume that there are some agents in each of the two possible categories. So to induce agents to supply labor, the labor market must pay wage w .⁵ Agents pursue entrepreneurship when the expected rewards equal or exceed w .

We assume, as does Lucas (1978), that our economy is populated by individuals who have identical abilities as workers, but who are endowed with different levels of entrepreneurial ability, θ , drawn from a continuous probability distribution $g(\theta)$, with support $[\theta_l, \theta_u]$. Entrepreneurial ability is a unique scarce resource (as in Schumpeter (1934)), which allows individuals to create and manage firms effectively.

To make matters interesting, we introduce a dynamic element by allowing agents to live for two periods. In the first period, an agent with endowment θ can choose home production

⁴For example, a country's legal and political origin is often treated as an instrument in this literature (Fisman and Sarria-Allende, 2004; Djankov et al., 2006; Dulleck et al., 2006; Barseghyan, 2008).

⁵Our assumptions imply that labor supply is perfectly *elastic*. This is a particularly transparent case for analysis. We might alternatively have chosen to analyze the equally transparent case of perfectly *inelastic* labor supply, as does Lucas (1978). However, that case rules out (by assumption) an interesting question we want to examine: Does entry regulation affect equilibrium employment? The intermediate case, with upward-sloping supply, adds considerable complication with no additional insight, so we do not pursue it here.

or work, in which case the payoff is w . Alternatively, she can become an entrepreneur, and receive a payoff $\pi_1(\theta)$ that is a known function of her entrepreneurial ability plus a mean-zero term, say ϵ (drawn from a known distribution), that reflects uncertainty that cannot be resolved until a potential entrepreneur actually opens her firm. To keep analysis simple, we let the value created by a firm take a constant-returns-to-scale Cobb-Douglas form in entrepreneurial ability and labor employed.⁶ The first-period payoff is

$$\pi_1(\theta) = \theta^{\frac{1}{2}} L^{\frac{1}{2}} - (wL + F) + \epsilon, \quad (1)$$

where L is the quantity of labor employed, and F is a fixed cost to opening the firm. In the second period, the agent retains her (now revealed) idiosyncratic term ϵ but faces no additional fixed cost. So

$$\pi_2(\theta) = \theta^{\frac{1}{2}} L^{\frac{1}{2}} - wL + \epsilon, \quad (2)$$

if she continues to operate. If, instead, our entrepreneur closes her firm (as she might if she has a particularly poor ϵ draw), she earns w .

Given this set up, it is easy to verify that an optimizing entrepreneur with ability θ and idiosyncratic draw ϵ earns

$$\pi_1(\theta, \epsilon) = \frac{\theta}{4w} - F + \epsilon, \quad (3)$$

and

$$\pi_2(\theta, \epsilon) = \begin{cases} \frac{\theta}{4w} + \epsilon & \text{if } \epsilon \geq w - \frac{\theta}{4w}, \\ w & \text{if } \epsilon \leq w - \frac{\theta}{4w}. \end{cases} \quad \text{and} \quad (4)$$

Clearly, the value of operating as an entrepreneur depends on one's ability θ , relative to the market wage w . Importantly for our purposes, the value of entrepreneurship depends also on the level of the fixed cost F , and so too, therefore, does the decision to become an entrepreneur. To illustrate, let ϵ be drawn from a uniform distribution with support $[-\epsilon_u, \epsilon_u]$. Then with a bit of algebra it is easy to confirm that a risk-neutral agent will choose to open a firm if, and only if

$$\left[\frac{\theta}{4w} - w \right] + \frac{1}{4\epsilon_u} \left[\left(\frac{\theta}{4w} - w \right) + \epsilon_u \right]^2 \geq F \quad (5)$$

(assuming a zero rate of time discounting). Expression (5) makes sense. The first bracketed term on the left-hand side is expected pure profit from operating in the first period. In a one-period model, the entrepreneurship decision would hinge solely on whether this term was as large as the fixed entry cost. There is an option value, though, associated with continued operation in the second period, and this is represented by the second, strictly positive, term on the left-hand side of (5).

Let $\hat{\theta}$ be the value of θ that solves (5) with equality. Then the fraction of agents who become entrepreneurs is $1 - G(\hat{\theta})$, where $G(\cdot)$ is the c.d.f. for $g(\cdot)$. Inspection of (5) gives the intuitive result that $\hat{\theta}$ is strictly increasing in F ; the lower the entry cost, the lower is the threshold that induces agents to open firms. In turn, the lower the entry cost, the higher will be the fraction of agents who become entrepreneurs.

⁶As part of our effort to keep things clear, we have no capital here.

Labor market employment in our economy of course depends on the level of entrepreneurship. In particular, an entrepreneur with ability θ employs $\theta/4w^2$ workers in period 1, and employs that same number in period 2 if she continues operation. So in our economy, a decrease in F increases employment (while decreasing the number of agents in home production). Notice that the firms that form as a consequence of a reduction in F will be operated by proprietors with relatively low levels of θ , and these firms will tend to be small, since labor demand is proportional here to entrepreneurial ability θ .

Finally, we note that the probability of firm survival in period 2 is also related to θ . In particular, a bit of algebra can be used to confirm that a firm's survival probability is

$$s(\theta) = \frac{\theta}{8w\epsilon_u} + \frac{1}{2} - \frac{w}{2\epsilon_u}. \quad (6)$$

Firms with high-ability proprietors (which are also firms with relatively more employees here) have a higher survival rate. Given that the distribution of talent among operating firms is $g(\theta)/[1 - G(\hat{\theta})]$, *average* firm survival is

$$\bar{s} = \int_{\hat{\theta}}^{\theta_u} \left[\frac{\theta}{8w\epsilon_u} + \frac{1}{2} - \frac{w}{2\epsilon_u} \right] \frac{g(\theta)}{1 - G(\hat{\theta})} d\theta, \quad (7)$$

which can be written

$$\bar{s} = \frac{1}{8w\epsilon_u} E(\theta | \theta \geq \hat{\theta}) + \frac{1}{2} - \frac{w}{2\epsilon_u}. \quad (8)$$

A reduction in F results in a reduction in the cut-off $\hat{\theta}$, and this in turn reduces the mean ability level of entrepreneurs. The consequence is to reduce average firm survival.

To summarize, our model provides an intuitive set of predictions about the consequence of a *reduction* in the cost of firm entry, i.e., a reform of the sort enacted in Portugal:

1. Increased business formation: A higher proportion of agents become entrepreneurs.
2. Increased employment: A higher proportion of agents are employed; fewer are in home production.
3. All shifts occur *at the margin*: New firms will generally have entrepreneurs with relatively lower talent. These firms will generally be smaller, and will have lower survival probabilities than infra-marginal firms.

It is worth noting that our model has some predictions that differ in the short- and long-run. For example, in our set-up only young agents form firms, because only they can take advantage of the option value of continued operation in case the firm proves to be particularly successful.⁷ A short-run consequence of reduced entry cost would be to cause some older entrepreneurs to form new firms. These would be agents who were deterred by high entry costs when they were young, but now find entrepreneurship to be more attractive. So in the short run, the average age of new entrepreneurs rises. In the long run, though,

⁷In a more complete model, the relationship between age and entrepreneurship becomes more complicated. Experience can increase ability, but agents still face the need to recoup the fixed costs of starting a firm over a sustained period of operation, making entrepreneurship less attractive for older workers.

once again only young agents form firms.

Finally, we note that in our model artificial barriers that increase entry costs introduce inefficiency; these regulations induce some potential entrepreneurs to unnecessarily and inefficiently decide to work for others or remain out of the labor force. Empirical support of the model's key predictions thus constitutes some useful *prima facie* evidence in favor of the proposition that reduced entry regulation is socially useful. If, to the contrary, one were to show little impact of entry deregulation, there would be less reason to be concerned about entry regulation as a practical concern. Conceivably, one might even find that reduced entry regulation reduces employment if markets are non-competitive.⁸

4 The “On the Spot Firm” Program

In this section, we describe the Portuguese business registration reform and the setting in which it was implemented.

Prior to 2005, to meet the government requirements, an entrepreneur had to visit several different public agencies, complete 11 procedures, fill out 20 forms and documents, wait between 54 and 78 days and pay almost 2,000 Euro (approximately 13.5 percent of per capita gross national income). These numbers were high by international standards, making Portugal one of the least attractive countries in which to start a business.⁹ Pressure to reform this system increased as the country's economic performance deteriorated after 2000. Between 1996 and 2000, the economy had experienced a period of reasonably good growth (real GDP increased by approximately 4.0 percent annually), but from 2001 to 2005 growth fell to less than 1.0 percent.

In response to the poor administrative record, and weak economic performance, the new government elected in February 2005 decided to rethink the regulatory regime. In May 2005 a cross-departmental task force, the Office of Public Services and Reform, or *Unidade de Coordenação da Modernização Administrativa* (UCMA), was created to oversee modernization and simplification of public services. One of the first issues tackled by this office was the simplification of the process of starting a business. As a result, the UCMA implemented the “On the Spot Firm” program, or *Empresa na Hora* (ENH), with the goal of decreasing the time, cost and complexity of starting a business.

The program allows the registration of single-shareholder companies, private limited companies, partnerships or public limited companies in a single office, the one-stop shop. However, it does not apply to governmental firms or firms involved in industries which require special authorization, permits or industry-specific requirements.¹⁰ Within one hour, an entrepreneur receives a corporate taxpayer number, social security number, commercial

⁸See, e.g., Mankiw and Whinston (1986) for a general discussion of free entry and social efficiency.

⁹For example, in 2005 a member country OECD required on average 6.5 procedures and 19.5 days, costing approximately 6.8 percent of the GNI per capita (World Bank, 2006). Djankov et al. (2002) present similar figures. In 2005 Portugal ranked 113 out of 155 countries in the *Doing Business Ranking* (World Bank, 2006), and if we consider only the waiting time to complete the registration procedures, it ranked 74 out of 85 countries studied by Djankov et al. (2002).

¹⁰Appendix A.1 lists all the industries that have to be registered by traditional procedures. The non-eligible

registration, and declaration of business initiation. In order to increase the efficiency of the process, the UCMA developed standardized pre-approved articles of association, created lists of pre-defined firm names, and eliminated outdated start-up formalities such as the registration of company books and the legal obligation to provide public deeds.

The law for this administrative model was approved July 6, 2005 (with *Decreto-Lei 111/2005*), and at the same time six one-stop shops were piloted in four different counties, Coimbra, Aveiro, Moita and Barreiro. The shops generally took advantage of preexisting Trade Registry Offices and Business Formalities Centers by locating the shops in these facilities. Due to resource constraints, the program was not implemented simultaneously in all counties. Rather, over time the program expanded to other locations across the country. Table 1 presents the statistics on the timing and geographic variation of program adoption from 2005 to 2009.

There was some resistance against the adoption of the program, particularly from the Association of Notaries, which in 2007 threatened to take the “On the Spot Firm” program to court, but these efforts did not slow the program’s expansion. By the end of 2009, there were 164 one-stop shops located in 135 different counties (44 per cent of total counties).¹¹ Although, the location of these shops are fairly dispersed throughout Portugal, the program targeted larger and economically important counties. At the same time that the program was implemented, Portugal had local elections for county chief executives. Political affiliation does not seem to play a significant role in program adoption.¹²

Because of the “On the Spot Firm” program, Portugal was considered the top reformer in business entry in 2005–2006, and was rewarded with the European Enterprise Award in the Red Tape Reducing category by the European Commission.¹³ Portugal’s position on the international competitive rankings improved markedly, e.g., improving from the 74th to 40th percentile in the *World Bank Red Tape Percentile Index*, and rising to 33rd out of 155 countries in the annual *Doing Business Ranking*. Figure 1 compares the number and time of the procedures needed to register a firm before and after the introduction of the one-stop shop. After the reform, an entrepreneur could easily finish the registration process in one day, at a cost of approximately 360 Euro.¹⁴

Two features of the program and institutional environment deserve additional discussion. First, at the end of 2008 there were three concurrent procedures to register a firm: traditional procedures, the “On the Spot Firm” program, and an online company incorporation (*Empresa Online*).¹⁵ However, the online company registration was initially only accessible to lawyers,

industries are mainly in the finance, insurance and transportation sector.

¹¹Portugal is subdivided into 308 counties, which are approximately one quarter of the size of U.S. counties.

¹²Approximately 40 percent of the counties with one-stop shops had a chief executive that was from the government’s party (PS) or from the main opposition party (PSD).

¹³World Bank (2006), for example, noted the changes: “In Portugal, now one of the fastest economies for start-up, an entrepreneur using the new fast-track service simply chooses a pre-approved name from the registry’s website then goes to the one-stop shop to register the company,” (p. 9).

¹⁴The total cost is 360 Euro or 300 Euro if the company’s focus is information technology or research and development.

¹⁵This latter program was the result of an initiative from June 2006, in which the government launched a special regime for setting up companies via the Internet (*Decreto-Lei 125/2006*), with similar steps as the “On the Spot Firm” program.

solicitors and notaries with a digital certification, and subsequently it only became available to individuals with the citizens card (cartão do cidadão) and electronic certification in the last trimester of 2009.¹⁶ Our data do not include information on the type of procedure used by entrepreneurs to register their firms. However, according to official information, more than 70 percent of new firms were established through the “On the Spot Firm” program until 2008. Traditional procedures and on-line registration were used much less frequently because of the cost and time of the former and the unavailability of the latter. Second, any individual or firm, anywhere in Portugal or abroad, can establish a firm in any one-stop shop, regardless of the location of the company’s headquarters. Although firms are allowed to register in one county and operate in another, this is not the usual procedure, and discussions with government officials strongly suggested that the fraction of firms registering outside their county of operation was trivially small. In short, over the time period we study, 2000-2008, there was considerable variation across regions and time in business registration costs and procedures. We will exploit this variation in policy using the rich data described below.

5 Data and Descriptive Statistics

The data for our empirical analysis come from an extraordinary matched employer-employee dataset (Quadros de Pessoal or SISED - Sistema de Informação de Salários, Emprego e Duração do Trabalho), built using a mandatory survey submitted annually by firms with at least one employee to the Portuguese Ministry of Employment and Social Security. These data include information on an average of 227,000 firms and two million individuals per year, covering virtually all employees and firms in the Portuguese private sector. As individuals and firms are cross-referenced by a unique identifier, the data make it possible to match founders with their firms characteristics. Each year, firms report their year of constitution, location, industry classification, number of employees, number of establishments, initial capital, and ownership structure. At the individual level, the data provide information on gender, age, date of hire, education, occupation, working hours, and earnings.

We supplement these data with information from other sources. Information on the opening date of each one-stop shop was obtained from the Institute of Registration and Notarization (Instituto dos Registos e do Notariado) at the Ministry of Justice.¹⁷ County-level data on inhabitants, gross domestic product, and an industrial production index are from the National Statistic Office.

Our matched employer-employee data includes 177,595 start-ups established in eligible industries for which we have a precise founding date that lies between 2000 and 2008. We use these firms and their characteristics to study the impact of the “On the Spot Firm” program on firm entry and job creation. For these new firms, we identify the founders and their

¹⁶The citizens card is a non-mandatory document, introduced in February 2007 (*Lei 7/2007*) with the goal of allowing individuals to identify themselves when dealing with computerized services and to authenticate electronic documents.

¹⁷The complete list of counties with an one-stop shop by December 2009 and their respective opening date is provided in Appendix A.2.

background history. We exclude firms for which we could not identify at least one owner or the background history of the founder.¹⁸ We also restrict the sample to founders with age between 20 and 60. In total, we ended up with 139,868 founders of 94,586 new firms. This entrepreneurs sample is used to evaluate the impact of the program on firm survival.

In addition, we draw a 30 percent random sample of *all* individuals who were employees in eligible firms between the period of 2000 and 2008,¹⁹ within the same age range (20 to 60) and with known labor market histories. Then we merge the latter sample with the entrepreneurs sample. This allows us to compare characteristics of firm founders and non-founders in specific years. In total, we then have a sample of 5,071,627 individuals, of whom 33,958 are entrepreneurs. These data allow us to evaluate the impact of the program on the decision to become an entrepreneur.

Our basic empirical strategy, discussed below, is to compare county-level economic outcomes before and after the introduction of the one-stop shops. Table 2 provides various descriptive statistics for such counties. In first column we give statistics for the 12 months *prior* to the opening of the one-stop shop, in the second column we provide statistics for the 12 months *after* the opening, and the third column we record the difference. In general, the opening of a one-stop shop is associated with an increase in number of firms and jobs created (though this increase is statistically significant only in the former case). Firms established after the program introduction have fewer employees, and low subsequent probability of surviving in the first two years. In terms of the founder characteristics, after the program is introduced entrepreneurs are less likely to be male, are slightly older, and are less experienced (though the changes in the first two features are not statistically significant). To make the later point, we focused on two categories of entrepreneurs: “novice entrepreneurs,” who have not previously established a firm, but who do have previous labor experience, and “habitual entrepreneurs,” who have both previous entrepreneurial and labor experience. We notice that after the opening of one-stop shops, there is a three percent increase in the proportion of novice entrepreneurs and a two percent decline in habitual entrepreneurs.

6 Empirical Methodology and Results

In this section we present estimates of the impact of the Portuguese reform on entrepreneurial outcomes—entry, job creation, firm size and survival—using a simple differences-in-differences approach. Eligible firms and individuals in counties with one-stop shops constitute the “treatment group,” which is compared to a “control group” with *no* one-stop shop. More precisely, we estimate regressions in which the outcome variable of interest is estimated as a function of indicator variables that measure seasonal effects, county-time fixed effects, and of course our key indicator variable that equals one when the reform takes place within a county. Our identification strategy relies, of course, on an assumption that the program is not being

¹⁸For the employees, the data include some cases in which the record changes in gender and year of birth. We consider observations with multiple changes in the gender or year of birth to be errors, corresponding to individuals whose identification number was not recorded, or wrongly identified by the respondent. We drop individuals whose gender and year birth change in more than 70 percent of the total number of observations.

¹⁹We exclude the year 2001 because there are no data available for workers that year.

rolled out in a way that correlates with pre-existing trends in the dependent variables of interest. For example, our strategy would give misleading inferences if the one-stop shops were purposely introduced in locations that initially were experience rapid job growth. We conduct some analysis below that suggests our assumption is credible. As an initial check of this issue, though, we investigated whether “early adopting” counties (which established one-stop shops between July 2005 and June 2007) differed from “late adopting” counties (which established one-stop shops between July 2007 and December 2008) in terms of initial growth trends (2000 through 2004) of total sales, sales of new firms, number of firms, number of new firms and purchasing power. We find no significant difference between counties.

6.1 Firm Entry and Job Creation

We start by determining the effect of business registration reform on firm entry. Let Y_{cmy} be the number of newly formed firms per 100,000 inhabitants in county c , month m and year y . We estimate, for the 308 counties in Portugal and over the time period 2000 through 2008,

$$Y_{cmy} = \sum_{m=1}^{12} \alpha_m + \sum_{c=1}^{308} \sum_{y=2000}^{2008} \nu_{cy} + \delta I_{my} + \theta Z_{cmy} + \epsilon_{cmy} \quad (9)$$

where α_m controls for seasonal effects in firm entry,²⁰ ν_{cy} are county/year fixed effects, I_{my} is a monthly index of industrial production for the country, which further controls for economic activity, and, Z_{cmy} is the key variable of interest—an indicator variable that equals one at the opening month of the one-stop shop and all subsequent months (and zero otherwise). Notice that because of our inclusions of county/year fixed effects, identification here comes from comparison of firm entry rates within a particular county in months immediately before the introduction of the one-stop shop to the firm entry rates in the months immediately thereafter. Standard errors for this and all subsequent regressions are clustered at the county level.

Column (1) of Panel A in Table 3 presents estimates. Our theory leads us to expect the opening of one-stop shops to lead to an increase in firm entry, i.e., that the estimate of θ should be positive. In fact, we find that the introduction of the one-stop shop is associated with an increase in the number of new firms per 100,000 inhabitants of approximately two, which is an approximately 17 percent increase.

Panel B in Table 3 uses the same specification to examine the impact of the one-stop shop reform on initial employment. In this case, the dependent variable in (9) is the initial number of employees of start-ups at the county level (per 100,000 inhabitants). As predicted, the coefficient is positive; we estimate that the reform is associated with an increase in initial employment in new firms of 7 per 100,000, corresponding to an increase of approximately 22 percent.

As we have mentioned, the key to our identification strategy is an assumption that administrative decisions to the open one-stop shops in particular counties and time periods

²⁰As it turns out, entry is generally stronger in the first three months of a year, slower in the summer, stronger in early fall, and slower again in November and December.

are not correlated with existing trends in firm formation or economic growth within counties. As a means of checking this assumption we estimate the following variant of our baseline regression:

$$Y_{cmy} = \sum_{m=1}^{12} \alpha_m + \sum_{c=1}^{308} \sum_{y=2000}^{2008} \nu_{cy} + \delta I_{my} + \sum_{l=-10}^{11} \theta_l z_{lc} + \epsilon_{cmy} \quad (10)$$

where z_{lc} is a set of indicator variables for the 10 months *prior* to the opening of a one-stop shop and the 10 months *after* the opening of the one-stop shop. Thus, for instance, z_{-1} is equal to one in the month prior to the opening of the one-stop shop (otherwise 0), while z_1 is equal to one for the month after the opening of the one-stop shop (otherwise 0). The only exception is z_{11} , which is equal to one for month 11 and for the months that follow. We set θ_{-1} to 0 (i.e., let that be the “omitted” indicator variable). Figure 2 and 3 plots the coefficients θ_l , for firm entry and employment regressions, respectively.

Consider Figure 2. The coefficients on the “lags” are generally close to 0; using an F test, we cannot reject the hypotheses that $\hat{\theta}_{-10}, \dots, \hat{\theta}_{-1} = 0$. This suggests that on average, in the months leading up to the establishment of the one-stop shop there was *no* unusual trend in the establishment of new firms. This, in turn, gives us increased confidence in our identifying strategy. In contrast, in the month of the establishment of the one-stop shop, and the months that follow, we have positive and statistically significant coefficients, which we expect, given the estimates from our baseline regression (9). Our F test easily rejects the null hypothesis that these coefficients jointly equal 0. Figure 3 shows comparable results for our analysis of employment at newly established firms.

Some counties have more than one one-stop shop by the end of the sample period. We find that our results (here and in other specifications) do not qualitatively change when we exclude these counties from the “treatment” group. Another concern is that entrepreneurs in counties that border another county with a one-stop shop might cross borders to register in the other county, then operate in the county of residence of the entrepreneur. This would lead us to underestimate the causal effect of the one-stop shop on observed effects, because some one-stop shops would “treat” individuals in the “control” counties. Conversations with officials in the one-stop shop program indicated that it was very rare for entrepreneurs to register in a county other than their county of operation. We experimented with aggregating counties where this border-crossing was more likely, and these alternative specifications produce stronger results than those reported here.

6.2 Firm Characteristics

Our theory leads us to believe that any impact of the reform—the establishment of the one-stop shops—should be found in “marginal firms,” which in our theory are also small firms. So we estimate our key regression (9) but for firms categorized by the firm’s initial size. Columns (2) to (5) of Panel A in Table 3 report the coefficient when we consider the number of firms with one, two, three to five, and more than five employees, respectively. Our estimates indicate statistically significant increases in the number of newly established firms with two employees, and three to five employees, but no statistically significant increase in larger firms.

Next, the number of new firms is broken down into ten sectors: agriculture, construction, high-technology industries, low technology industries, utilities, wholesale retail, retail trade, services and communities.²¹ Table 4 presents the results for this analysis, again using (9). The estimated positive impact of the reform is found to operate in three sectors: agriculture, construction and retail trade.

6.3 Firm Survival

As we mention in our paper’s introduction, a distinctive feature of our data is the ability to examine the impact of the reform on the survival of newly-formed firms. Our approach is to examine the two-year survival rate, in which S_{iflcm_y} is firm survival for founder i in firm f , industry l , county c , month m and year y . This dependent variable is 1 if the start-up is still operating after two years, and 0 if not. It is taken to be a function of observables, as follows:

$$S_{iflcm_y} = \sum_{m=1}^{12} \alpha_m + \sum_{c=1}^{308} \sum_{y=2000}^{2008} \nu_{cy} + \sum_{l=1}^{29} \lambda_l + \omega S_f + \tau X_i + \theta Z_{cm_y} + \delta I_{m_y} + \epsilon_{iflcm_y}, \quad (11)$$

where again we have indicators for month, year-county fixed effects, and now also two-digit industry fixed effects for 29 categories λ_l . We also have a variable that gives the firm’s initial size, S . We include also a vector founder characteristics X : gender, which equals 1 for men, 0 for women; four indicator variables for the founder’s age, partitioned at 20, 30, 40, and 50; an “industry experience” variable which equals 1 for founders that previously work on the same four-digit industry digit code, 0 otherwise; and education, which is taken to be “very low” for those never completing elementary school, “low” for those that attended junior high school, “medium” for those with a high school diploma or equivalent, and “high” for those reporting bachelor’s degree or more advanced degree. In reporting the estimated coefficients, our omitted categories are founders aged 20–29 and with “very low” education.

Table 5 gives estimates. We notice that in general start-up firm survival is higher for initially larger firms, and for firms founded by experienced individuals, by men (in comparison to women), by relatively older individuals, and by relatively well educated individuals. Importantly, there is a substantial estimated impact associated with the one-stop shop program. In comparison with other start-ups, firms established in the months after the availability of the one-stop shop program had two year survival rates that are approximately four percentage points lower.

²¹We use the definition of OECD (2002) to divide firms into high-technology and low-technology industries. “High-technology industries” include the following sectors: pharmaceuticals, office and computing machinery, radio, TV and communication equipment, medical, precision and optical equipment, aircraft and spacecraft, chemicals excluding pharmaceuticals, machinery and equipment, electrical machinery and apparatus, motor vehicles and trailers, railroad and transport equipment. “Low-technology industries” include coke, refined petroleum products and nuclear fuel, rubber and plastic products, other non-metallic mineral products, basic metals, fabricated metal products except machinery and equipment, building and repairing ships and boats, food products, beverage and tobacco, textile and textile products, leather and footwear, wood, pulp, paper products, printing and publishing, and recycling. We acknowledge that this is a rough taxonomy; the low-technology category could include some technologically progressive firms and vice versa.

As in the analyses presented above, we also estimate the impact of the one-stop shop program on our survival variable, using estimates for lag and lead months. Figure 4 plots the coefficients on the lag and lead indicator variables, which were constructed using the same methodology as presented in Section 6.1. As can be seen in Figure 4, the fraction of surviving start-ups is close to zero before the opening of the one-stop shop and then it becomes jointly significantly negative from months 0 to > 10 .

6.4 Entrepreneur Characteristics

In this last section, we explore the effect of the one-stop program by estimating an equation in which the dependent variable is the probability that *any* individual in our data founds a start-up firm in a given year. Specifically, we start with the simple linear probability model:

$$E_{icy} = \sum_{y=2000}^{2008} \beta_y + \sum_{c=1}^{308} \gamma_c + \sum_{n=1}^{30} \delta G_{ny} + \theta Z_{cy} + \tau X_i + \epsilon_{icy} \quad (12)$$

where E_{icy} is an indicator variable that equals 1 if individual i establishes a firm in county c and year y , and 0 if that individual does not open a firm. We have also control for economic activity with a measure of gross domestic product (GDP), G_n , for 30 NUTS (“Nomenclature of Unit for Territorial Statistics”) regions defined for Portugal. We include also a vector of individual characteristics X_i (measures of age, gender, and education). Finally, Z_{cy} is an indicator variable equal to 1 if there is a one-stop shop in county c in year y for at least part of the year.²² We remind the reader that these results are based on a 30 percent random sample of the database in which there are more than five million individuals, but fewer than 34,000 of these individuals transition into entrepreneurship. In other words, less than 0.68 percent (0.0068) of the sample starts a new firm; our econometric estimates should be evaluated in light of this feature of the sample.

The estimates for this specification are presented in Column (1) of Table 6. Given results above, we are not surprised that the presence of a one-stop shop is associated with an increase in the probability an individual becomes an entrepreneur. Given the small fraction of the sample that ever becomes an entrepreneur, the coefficient on the One-Stop Shop dummy is associated with an marginal increase of about 9.2 percent. As for demographic and education variables, we can infer that male, middle aged (age between 30 and 39), and well-educated individuals are relatively more likely to transition into entrepreneurship than are other individuals.

Our model predicts that entry regulation reform leads to an increase in the entry of “marginal entrepreneurs.” To evaluate that claim we interact each of the demographic and educational variables with Z_{cy} (our indicator that the reform has occurred in the county in that year) and add these interactions to Equation (12). The coefficient estimates are reported in Column (2) of Table 6. These results suggest that the entrepreneurs induced into the market by the establishment of the one-stop shop are from demographic categories that were previously *least* likely to initially be entrepreneurs. Relative to the omitted category

²²Our data for this regression is annual, so we cannot look at within-year effects as in the analyses above.

(very low educated individuals), the fraction of highly educated entrepreneurs decreases by 0.4 percent. Similarly, marginal entrants appear to be older (individuals aged 40–50 and 50–60) and are less likely to be male.

These results, combined with the evidence presented above—that regulation reform is associated with the entry of firms that are less likely to survive to the two-year mark, provide some support for the argument that the reduced entry regulation disproportionately induced marginal, relatively low-ability individuals to establish businesses.

7 Conclusion

This paper uses Portuguese micro-level data to analyze the effects of a program that substantially reduced entry regulation for business. The reform, which introduced one-stop shops in counties throughout the country, appears to have had meaningful effects on start-up entry, performance, and survival.

By comparing counties with and without one-stop shops, we find that the program increases the number of start-ups in eligible industries by approximately 17.2 percent. This increase is disproportionately driven by entrepreneurs who are relatively older, more female and less educated. Based on our estimates, the reform is associated with an increase in seven new jobs per month per 100,000 inhabitants (a 21.7 percent increase). These jobs are created primarily in the agricultural, retail trade and construction sectors. We also find that the start-ups created in response to the reform are relatively smaller and less likely to survive in their first two years than firms founded in the absence of the reform.

We view the results as entirely consistent with the theoretical model of entrepreneurship set out in this paper. Inspired by the seminal work of Lucas (1978), we show that artificial entry costs deter entrepreneurship *at the margin*. Reform, i.e., the reduction of entry costs, is predicted to affect entrepreneurship and job formation in just the way we in fact observe. Our results, then, can be seen as broadly consistent with the “public choice” approach to entry regulation, which emphasizes the inefficiencies associated with entry regulation, and argues for the relaxation of such regulation.

In the Portuguese case, the reform was substantial. Portugal undertook one of the most complete and thorough deregulation efforts of any country in Western Europe, moving up 80 places in the World Bank’s Doing Business index and winning international accolades for the government in the process. As it was implemented across the country, this reform appears to have induced statistically significant increases in firm formation and job creation.

Having said all that, a more detailed examination of the kind of firms and jobs created by this reform suggests a more nuanced picture of its effects. The new firms whose entry is associated with reform are decidedly low-tech, headed by inexperienced entrepreneurs, and are less likely to survive than firms formed when barriers were higher. Portugal’s high entry barriers were a significant impediment to low quality firms and marginal entrepreneurs, but apparently represented less of an impediment to high quality firms. The overall social gains resulting from the reform are limited by the quality of the firms it has created, at least so

far. Reform has brought benefits, but it is hardly the magic bullet some policymakers may have anticipated.

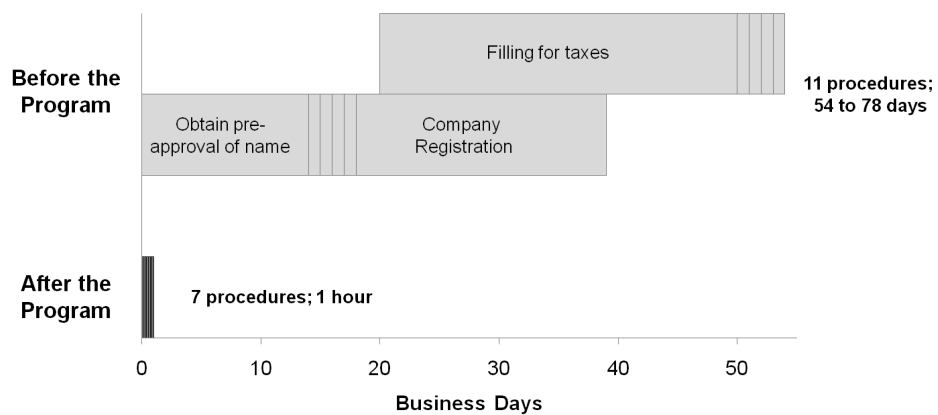
This conclusion comes with some obvious caveats. We have deliberately adopted a conservative empirical specification that attempts to control as completely as possible for differential trends in firm creation at the county level; the conservative nature of our approach could bias our estimates downward. In addition, this reform was implemented in a difficult macroeconomic context. Portugal emerged from recession in 2003, but growth since then has been weak, consumption and investment have remained depressed, and unemployment has been high. If and when the macroeconomic environment improves, the new administrative regime could have a stronger effect on firm and job creation than is evidenced by our analysis. Unfortunately, given the depth of Portugal's current economic difficulties, it may be some time before scholars will be in a position to evaluate this possibility.

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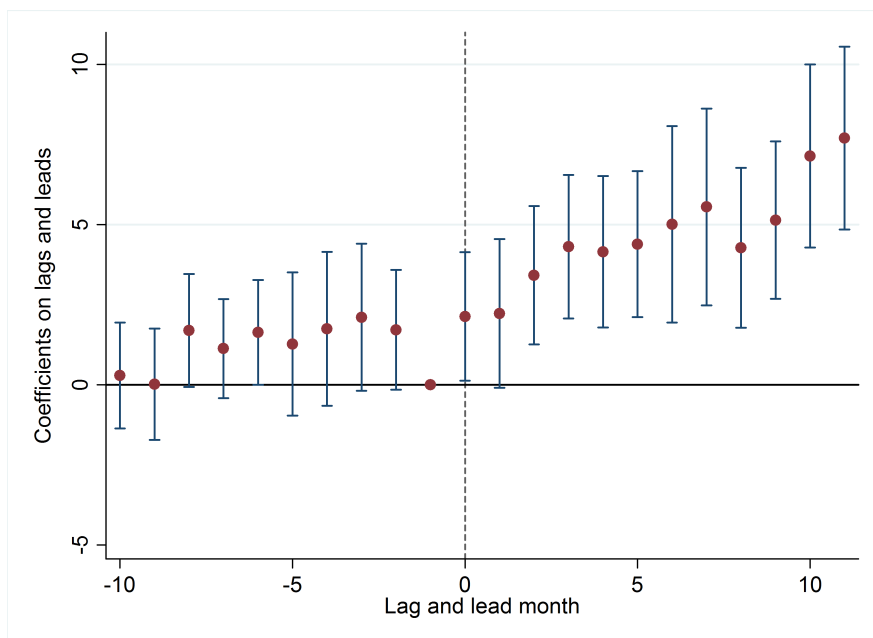
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Figure 1: Start-up Procedures in Portugal, Before and After the “On the Spot Firm” Program



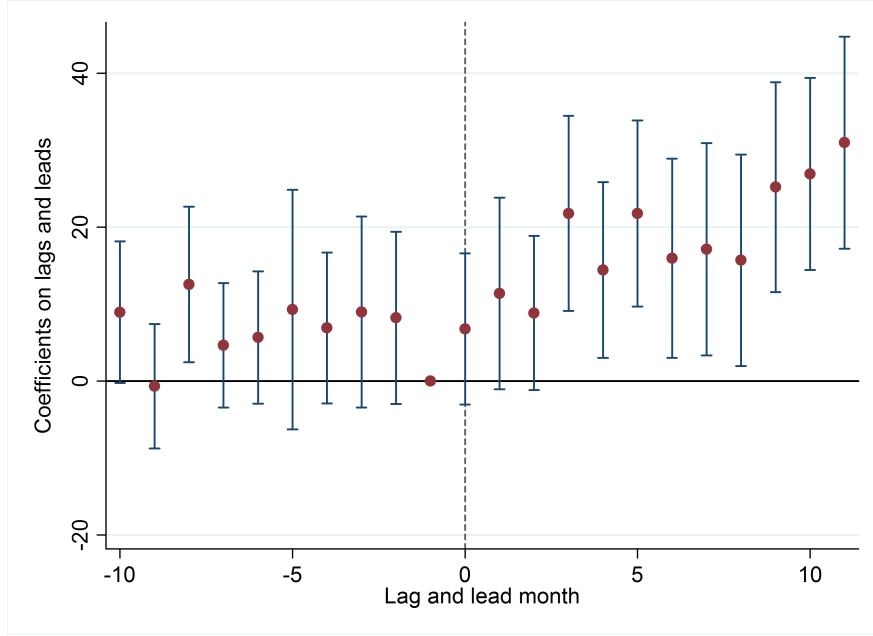
Note: The registration procedures are lined up sequentially. Business days required to complete each procedure are measured against the horizontal scale.

Figure 2: Coefficients on the Program Month Dummies for Firm Entry



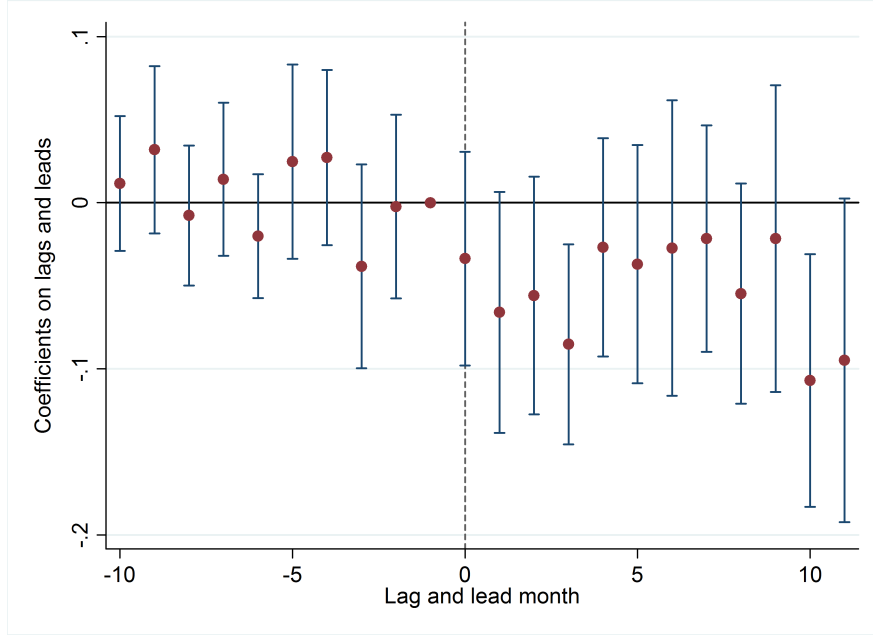
Note: The figure plots the coefficients θ_l from Equation (10), with a dependent variable, number of firms established in a county per 100,000 inhabitants. Vertical lines are the 95 percent confidence intervals for the coefficients. The F statistic for *pre* period is 1.122 (p-value is 0.347); F test statistic for *post* period is 2.839 (p-value is 0.0011).

Figure 3: Coefficients on the Program Month Dummies for Job Creation



Note: The figure plots the coefficients θ_l from Equation (10), with a dependent variable, number of employees at newly-established firms in a county per 100,000 inhabitants. Vertical lines are the 95 percent confidence intervals for the coefficients. The F statistic for *pre* period is 1.578 (p-value is 0.121); F test statistic for *post* period is 2.889 (p-value is 0.0085).

Figure 4: Coefficients on the Program Month Dummies for Two Year Survival



Note: The figure plots the coefficients θ_l from Equation (11), with an indicator dependent variable equal to 1 if the firm survived two years. Vertical lines are the 95 percent confidence intervals for the coefficients. The F statistic for *pre* period is 0.65 (p-value is 0.756); F test statistic for *post* period is 3.69 (p-value is less than 0.001).

Table 1: One-Stop Shop Program Adoption

	2005	2006	2007	2008	2009	Total
Number of shops	20	28	31	36	49	164
Number of counties implementing	13	24	28	34	46	135
Number of counties >1 shop	6	3	1	2	0	12

Note: The following counties have more than one one-stop shop: Aveiro, Braga, Castelo Branco, Coimbra, Guarda, Leiria, Lisbon, Loulé, Odivelas, Porto, Setúbal, and Viseu.

Table 2: Descriptive Statistics

	Before	After	Diff
A. Firm Entry and Job Creation			
Number of Firms	191.0 (8.916) 57	214.7 (10.89) 57	23.69* (14.07) 114
Job Creation	600.3 (39.33) 57	630.8 (40.66) 57	30.45 (56.57) 114
B. Firm Characteristics			
Size	3.735 (0.0816) 6186	3.493 (0.0600) 6981	-0.241** (0.101) 13167
2 Year Survival	0.833 (0.0109) 1165	0.801 (0.0108) 1379	-0.0329** (0.0153) 2544
C. Entrepreneur Characteristics			
Proportion Male	0.667 (0.00599) 6186	0.659 (0.00567) 6981	-0.00714 (0.00825) 13167
Average Age	35.82 (0.114) 6186	35.86 (0.109) 6981	0.0375 (0.158) 13167
Proportion Novice	0.548 (0.00646) 5943	0.581 (0.00604) 6668	0.0336*** (0.00884) 12611
Proportion Habitual	0.168 (0.00485) 5943	0.152 (0.00439) 6668	-0.0158** (0.00654) 12611

Note: The table reports various descriptive statistics for counties that opened one-stop shops in a 12-month window before the shop opened and a 12-month window after the shop opened. Robust standard errors are in parentheses and the number of observations is presented below. *** denotes statistical significance at 1%, ** significance at 5% , * significance at 10%.

Table 3: Impact of “On the Spot Firm” Program on Entry and Job Creation

Panel A. Firm Entry					
	All Firms	One Employee	Two Employees	Three to Five Employees	More than Five Employees
	(1)	(2)	(3)	(4)	(5)
One-Stop Shop ($Z = 1$)	1.999*** (0.655)	0.544 (0.426)	0.585** (0.241)	0.611** (0.262)	0.259 (0.212)
Econ. Activity Index	0.156** (0.0755)	0.0327 (0.0285)	0.0544*** (0.0108)	0.0541** (0.0228)	0.0152 (0.0194)
Observations	32648	32648	32648	32648	32648
Adjusted R-squared	0.654	0.406	0.346	0.390	0.307
Panel B. Job Creation					
	Initial Employment				
	(1)				
One-Stop Shop ($Z = 1$)	6.616* (3.386)				
Econ. Activity Index	0.545* (0.316)				
Observations	32648				
Adjusted R-squared	0.496				

Note: Panel A and B report the estimates of coefficients in (9). The dependent variable is the number of new firms established per 100,000 inhabitants. Columns (2) to (5) conducts the analysis for firms by initial size. In Panel B, the dependent variable is the number of employees at start-up firms per 100,000 inhabitants. Month and county-year fixed effects are included but not reported. Standard errors clustered at the county level are in parentheses. *** denotes statistical significance at 1%, ** significance at 5%, * significance at 10%.

Table 4: Impact of “On the Spot Firm” Program on Firm Industry

	Agriculture	High-Technology Industries	Construction	Low-Technology Industries	Utilities	Wholesale Trade	Retail Trade	Real Estate	Services	Communities
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
One-Stop Shop ($Z = 1$)	0.735*** (0.200)	0.0559 (0.0462)	0.745*** (0.250)	0.215 (0.180)	-0.0109 (0.0210)	0.191 (0.131)	0.486* (0.273)	-0.0306 (0.0928)	-0.416 (0.290)	-0.118 (0.122)
Econ. Activity Index	0.00690 (0.00871)	0.00407*** (0.00120)	0.0365*** (0.0117)	0.0231 (0.0166)	0.000136 (0.000658)	0.0169 (0.0112)	0.0291 (0.0189)	0.00205 (0.00324)	0.0277** (0.0111)	0.00462 (0.00289)
Observations	32648	32648	32648	32648	32648	32648	32648	32648	32648	32648
Adj R-squared	0.110	0.083	0.378	0.310	0.007	0.181	0.317	0.090	0.338	0.105

Note: The table reports estimated coefficients for (9). The dependent variable is the number of new firms established per 100,000 inhabitants divided in ten industries: agriculture, construction, high-technology industries, low technology industries, utilities, wholesale retail, retail trade, services and communities.

We use the definition from OECD (2002) to classify high and low technology industries. Month and county-year fixed effects are included but not reported. Standard errors clustered at the county level are in parentheses. *** denotes statistical significance at 1%, ** significance at 5%, * significance at 10% .

Table 5: Impact of “On the Spot Firm” Program on Survival

	Two Year Survival
	(1)
One-Stop Shop ($Z = 1$)	-0.0444** (0.0209)
Ind	0.0153*** (0.00303)
Size	0.00302*** (0.000259)
Gender	0.00765*** (0.00229)
Age 30-39	0.0268*** (0.00308)
Age 40-49	0.0321*** (0.00311)
Age 50-60	0.0410*** (0.00398)
Low education	0.00481 (0.00343)
Medium education	0.00612 (0.00458)
High education	0.0408*** (0.00482)
Econ. Activity Index	0.000168 (0.000750)
Constant	0.911*** (0.0768)
Observations	108637
Adjusted R-squared	0.044

Note: The table reports estimated coefficients for (11). The dependent variable is two year survival (1 if the firm survived, 0 if not). Month, county-year, and industry fixed effects (two digit level) are included but not reported. Standard errors clustered at the county level are in parentheses. *** denotes statistical significance at 1%, ** significance at 5% , * significance at 10%.

Table 6: Impact of “On the Spot Firm” Program on Type of Entrepreneurs

	Decision of Becoming an Entrepreneur	
	(1)	(2)
One-Stop Shop ($Z = 1$)	0.000623** (0.000307)	
Gender	0.00358*** (0.000310)	0.00379*** (0.000285)
Age 30-39	0.00213*** (0.000186)	0.00204*** (0.000210)
Age 40-49	0.00142*** (0.000163)	0.00126*** (0.000208)
Age 50-60	0.000206 (0.000179)	-0.000121 (0.000228)
Low education	0.00240*** (0.000239)	0.00235*** (0.000265)
Medium education	0.00580*** (0.000629)	0.00590*** (0.000594)
High education	0.00877*** (0.000777)	0.00926*** (0.000747)
(Gender) $\times Z$		-0.000790*** (0.000246)
(Age 30-39) $\times Z$		0.000368 (0.000243)
(Age 40-49) $\times Z$		0.000582** (0.000269)
(Age 50-60) $\times Z$		0.00114*** (0.000379)
(Low educ) $\times Z$		0.000132 (0.000195)
(Medium educ) $\times Z$		-0.000178 (0.000160)
(High educ) $\times Z$		-0.000461*** (0.000168)
Gdp	2.96e-07*** (7.71e-08)	3.33e-07*** (8.82e-08)
Constant	-0.00975*** (0.000441)	-0.00964*** (0.000431)
Observations	5071627	5071627
Adjusted R-squared	0.003	0.003

Note: The table reports estimated coefficients for (12). The dependent variable equals 1 if the individual is an entrepreneur, 0 otherwise. Year and county fixed effects are included but not reported. Standard errors clustered at the county level are in parentheses. *** denotes statistical significance at 1%, ** significance at 5%, * significance at 10%.

A Appendix

A.1 List of Non-Eligible Industries

Division	Non-eligible industries and occupations
Manufacturing	Arms and Ammunitions
Transportation and Communication	Railroad Transportation Local and Suburban Transit and Interurban Passenger Transportation Motor Freight Transportation Water Transit Transportation by Air Transportation Services (e.g. storage, administrative services) Telephone Communications Television Broadcasting Stations Journalism
Retail Trade	Automotive Dealers Gasoline Service Stations Catering Drug Stores Retail of Arms and Ammunitions Retail of Electric Energy
Finance, Insurance, And Real Estate	Commercial Banks Security Brokers and Dealers Pension Funds Insurance Carriers Insurance Agents Investment Offices Real Estate
Services	Hotels, Boarding Houses and Spas Detective and Guard Services Automobile Inspections Professional Sports Clubs Gambling Establishments and Casinos Legal Services Professional Schools Employment Services/ Agencies Vocational Training Driving Schools Zoological Parks Accounting, Auditing, and Bookkeeping Services Management Services and Holdings Towing Services Car Rental Services Car Inspection Service Funeral Houses
Public Administration	All

A.2 One-Stop Shop Opening Dates

One-Stop Shop	County	Openning Date	Closing Date
CRC Aveiro	Aveiro	Jul 14, 2005	
CRC Barreiro	Barreiro	Jul 14, 2005	
CRC Coimbra	Coimbra	Jul 14, 2005	
CRC Moita	Moita	Jul 14, 2005	
CFE Aveiro	Aveiro	Jul 14, 2005	
CFE Coimbra	Coimbra	Jul 14, 2005	
CRC Beja	Beja	Nov 15, 2005	
CRC Braga	Braga	Nov 15, 2005	
CRC Bragança	Bragança	Nov 15, 2005	
CRC Guarda	Guarda	Nov 15, 2005	
CRC V.N. Gaia	Vila Nova de Gaia	Nov 15, 2005	
CFE Braga	Braga	Nov 15, 2005	
CRC Loulé	Loulé	Dec 27, 2005	
CRC Sintra	Sintra	Dec 27, 2005	
CRC Viseu	Viseu	Dec 27, 2005	
RNPC Lisboa	Lisboa	Dec 27, 2005	
CFE Lisboa I	Lisboa	Dec 27, 2005	Feb 8, 2008
CFE Lisboa II	Lisboa	Dec 27, 2005	
CFE Loulé	Loulé	Dec 27, 2005	
CFE Viseu	Viseu	Dec 27, 2005	
CRC Évora	Évora	Jan 27, 2006	
CRC Leiria	Leiria	Jan 27, 2006	
CRC Santarém	Santarém	Jan 27, 2006	Jan 20, 2009
CRC Viana do Castelo	Viana do Castelo	Jan 27, 2006	
CFE Leiria	Leiria	Jan 27, 2006	
CRC Castelo Branco	Castelo Branco	Feb 24, 2006	
CRC Portalegre	Portalegre	Feb 24, 2006	
CRC Setúbal	Setúbal	Feb 24, 2006	
CRC Vila Real	Vila Real	Feb 24, 2006	
CFE Setúbal	Setúbal	Feb 24, 2006	
CRC Porto	Porto	Mar 31, 2006	
CFE Porto	Porto	Mar 31, 2006	
CFE Funchal	Funchal	Apr 11, 2006	
CRC Gondomar	Gondomar	Apr 28, 2006	
CRC Ponta Delgada	Ponta Delgada	Apr 28, 2006	
CRC Faro	Faro	May 31, 2006	
CRC Angra do Heroísmo	Angra do Heroísmo	Jun 1, 2006	
CRC Maia	Maia	Jun 1, 2006	
1ªCRPC Guimarães	Guimarães	Sep 29, 2006	
CRPC São João da Madeira	São João da Madeira	Sep 29, 2006	
CRPC Bombarral	Bombarral	Oct 31, 2006	
CRC Lisboa	Lisboa	Oct 31, 2006	
CRPC Odivelas	Odivelas	Oct 31, 2006	
CRPC Vila Franca de Xira	Vila Franca de Xira	Oct 31, 2006	
CRC Cascais	Cascais	Nov 30, 2006	

One-Stop Shop	County	Openning Date	Closing Date
CRPC Portimão	Portimão	Nov 30, 2006	
CRPC Chaves	Chaves	Nov 30, 2006	
CRPC V.N. de Cerveira	Vila Nova de Cerveira	Dec 4, 2006	
CRPC Celorico de Basto	Celorico de Basto	Mar 30, 2007	
CRPC Grândola	Grândola	Mar 30, 2007	
CRPC Lamego	Lamego	Mar 30, 2007	
CRPC Vila do Conde	Vila do Conde	Mar 30, 2007	
CRPC Covilhã	Covilhã	Apr 30, 2007	
CRPC Estremoz	Estremoz	Apr 30, 2007	
CRPC Monção	Monção	Apr 30, 2007	
CRPC V.N. Famalicão	Vila Nova de Famalicão	Apr 30, 2007	
CRPC Águeda	Águeda	May 31, 2007	
CRPC Elvas	Elvas	May 31, 2007	
CRPC Seia	Seia	May 31, 2007	
CRPC Torres Vedras	Torres Vedras	May 31, 2007	
CRPC Alcácer do Sal	Alcácer do Sal	Jun 29, 2007	
CRPC Figueira da Foz	Figueira da Foz	Jun 29, 2007	
CRPC Santiago do Cacém	Santiago do Cacém	Jun 29, 2007	
CRPC V.R. Santo António	Vila Real de Santo António	Jun 29, 2007	
Postos dos Registos	Lisboa	Jul 6, 2007	
CRPC Mirandela	Mirandela	Sep 27, 2007	
CRPC Oliveira do Bairro	Oliveira do Bairro	Sep 28, 2007	
CRPC Sertã	Sertã	Sep 28, 2007	
CRPC Vila Nova de Foz Côa	Vila Nova de Foz Côa	Sep 28, 2007	
CRPC Fornos de Algodres	Fornos de Algodres	Oct 31, 2007	
CRPC Pombal	Pombal	Oct 31, 2007	
CRPC Tomar	Tomar	Oct 31, 2007	
1ºCNCE Porto	Porto	Oct 31, 2007	
CRPC Horta	Horta	Nov 30, 2007	
CRPC Abrantes	Abrantes	Nov 30, 2007	
CRPC Caldas da Rainha	Caldas da Rainha	Nov 30, 2007	
CRPC Lagos	Lagos	Nov 30, 2007	
CRPC Montemor-O-Novo	Montemor-O-Novo	Nov 30, 2007	
Loja do Cidadão Odivelas	Odivelas	Dec 17, 2007	
CRPC Aljustrel	Aljustrel	Jan 31, 2008	
CRPC Fafe	Fafe	Jan 31, 2008	
CRPC Valongo	Valongo	Jan 31, 2008	
1ªCRC Loures	Loures	Feb 29, 2008	
CRC Espinho	Espinho	Feb 29, 2008	
CRC Oliveira de Azeméis	Oliveira de Azeméis	Feb 29, 2008	
CRC Tondela	Tondela	Feb 29, 2008	
1ªCRC Almada	Almada	Mar 31, 2008	
CRC Figueira de Castelo Rodrigo	Figueira de Castelo Rodrigo	Mar 31, 2008	
CRC Idanha-A-Nova	Idanha-A-Nova	Mar 31, 2008	
CRC Moimenta da Beira	Moimenta da Beira	Mar 31, 2008	
CRC Aljezur	Aljezur	Apr 30, 2008	
CRC Almeida	Almeida	Apr 30, 2008	
1ºCNCE Castelo Branco	Castelo Branco	Apr 30, 2008	
CRC Vila Verde	Vila Verde	Apr 30, 2008	
CRC Alfândega da Fé	Alfândega da Fé	May 30, 2008	
CRC Macedo de Cavaleiros	Macedo de Cavaleiros	May 30, 2008	
CRC Odemira	Odemira	May 30, 2008	
CRC Ovar	Ovar	May 30, 2008	
CRC Ílhavo	Ílhavo	Jun 30, 2008	
CRC São João da Pesqueira	São João da Pesqueira	Jun 30, 2008	

One-Stop Shop	County	Openning Date	Closing Date
1ªCNCE. Guarda	Guarda	Jun 30, 2008	
1ªCNCE Matosinhos	Matosinhos	Jun 30, 2008	
CRC Alcobaça	Alcobaça	Sep 30, 2008	
CRC Felgueiras	Felgueiras	Sep 30, 2008	
CRC Mora	Mora	Sep 30, 2008	
CRC Valença	Valença	Sep 30, 2008	
CRC Óbidos	Óbidos	Oct 31, 2008	
CRC Ponte da Barca	Ponte da Barca	Oct 31, 2008	
CRC Ponte de Lima	Ponte de Lima	Oct 31, 2008	
CRC Trofa	Trofa	Oct 31, 2008	
CRC Cantanhede	Cantanhede	Nov 28, 2008	
CRC Montalegre	Montalegre	Nov 28, 2008	
CRC Moura	Moura	Nov 28, 2008	
CRC Ponte de Sôr	Ponte de Sôr	Nov 28, 2008	
CRC Santo Tirso	Santo Tirso	Nov 28, 2008	
CRP Serpa	Serpa	Feb 6, 2009	
CRP Alcanena	Alcanena	Feb 20, 2009	
CRP Caminha	Caminha	Feb 20, 2009	
CRP Ferreira do Zêzere	Ferreira do Zêzere	Feb 20, 2009	
CRP Marco de Canaveses	Marco de Canaveses	Mar 17, 2009	
CRP Mortágua	Mortágua	Mar 17, 2009	
CRP Murça	Murça	Mar 17, 2009	
CRP Borba	Borba	Mar 17, 2009	
CNCE de Viseu	Viseu	Apr 8, 2009	
CRP Ferreira do Alentejo	Ferreira do Alentejo	Apr 8, 2009	
CRP Pedrogão Grande	Pedrogão Grande	Apr 8, 2009	
CRP Valpaços	Valpaços	Apr 8, 2009	
CRP Penafiel	Penafiel	Apr 8, 2009	
CRP Murtosa	Murtosa	Apr 8, 2009	
CRP Arganil	Arganil	May 4, 2009	
CRP Mafra	Mafra	May 4, 2009	
CRP Armamar	Armamar	May 4, 2009	
CRP Arouca	Arouca	May 4, 2009	
CRP Vila Flor	Vila Flor	May 4, 2009	
CRP Azambuja	Azambuja	May 29, 2009	
CRP Cadaval	Cadaval	May 29, 2009	
CRP Sobral de Monte Agraço	Sobral de Monte Agraço	May 29, 2009	
CRP Resende	Resende	May 29, 2009	
CRP Castanheira de Pêra	Castanheira de Pêra	Jun 18, 2009	
CRP Batalha	Batalha	Jun 25, 2009	
CRP Ourique	Ourique	Jun 25, 2009	
CRP Rio Maior	Rio Maior	Jun 25, 2009	
CRP Barcelos	Barcelos	Jul 9, 2009	
CRP Tavira	Tavira	Aug 31, 2009	
CRP Arruda dos Vinhos	Arruda dos Vinhos	Sep 7, 2009	
CRP Cartaxo	Cartaxo	Sep 7, 2009	
CRP Vimioso	Vimioso	Sep 7, 2009	
CRP Peniche	Peniche	Sep 7, 2009	
CRP Seixal	Seixal	Sep 7, 2009	
CRP Vouzela	Vouzela	Sep 7, 2009	
CRP Campo Maior	Campo Maior	Sep 7, 2009	
CRP Entroncamento	Entroncamento	Oct 2, 2009	
CRP Freixo de Espada à Cinta	Freixo de Espada à Cinta	Oct 2, 2009	
CRP Marinha Grande	Marinha Grande	Oct 2, 2009	
CRP Oliveira do Hospital	Oliveira do Hospital	Oct 2, 2009	

One-Stop Shop	County	Openning Date	Closing Date
CRP Alenquer	Alenquer	Oct 27, 2009	
CRP Lourinhã	Lourinhã	Oct 27, 2009	
CRP Nazaré	Nazaré	Oct 27, 2009	
SIR- Soluções Integradas de Registo	Lisboa	Oct 27, 2009	
CRP Póvoa de Varzim	Póvoa de Varzim	Nov 20, 2009	
CRP Belmonte	Belmonte	Nov 20, 2009	
CRP Nelas	Nelas	Nov 20, 2009	
CRP Mangualde	Mangualde	Nov 20, 2009	
Espaço Registos do Areeiro	Lisboa	Dec 2, 2009	

Note: CFE is Centro de Formalidades de Empresas; CRC is Conservatória do Registo Comercial; CRP is Conservatória do Registo Predial; CRCPC is Conservatória do Registo Civil, Predial e Comercial; CRPC Conservatórias do Registo Predial e Comercial; CNCE is Cartório de Competência Especializada; RNPC is Registo Nacional de Pessoas Colectivas.

A.3 Data and Construction of Variables

The data in this paper come from the matched employer- employee dataset (Quadros de Pessoal or SISED - Sistema de Informação de Salários, Emprego e Duração do Trabalho), which is a mandatory survey submitted annually in October by all firms with at least one employee. This database collects information on an average of 227,000 firms and two million individuals per year, covering virtually all employees and firms in the Portuguese private sector.

This database is generally available annually from 1982 onward, however we restrict our analysis to the period between 2000 and 2008. Data are unavailable for the year 2001.

The database contains three related sets of records: one at the firm level, other at the establishment level and the last one at the employee level. Employees, firms and establishments are cross referenced by a unique identifier. Each year, firms report their year of incorporation, location (concelho or county where the main offices are located), main industry, number of employees, number of establishments, initial capital, ownership structure and sales. At the establishment level, firms report the number of employees, location, and main industry. At the individual level, the database contains information on gender, age, date of hire, education, occupation, working hours, and October's earnings. However, the employee records include redundant data or data with frequent changes in gender and/or year of birth for individual employees. We consider these observations to be errors, corresponding to individuals whose identification number was not inserted or wrongly identified by the respondent. We drop individuals whose gender and year birth change in more than 70 percent of the total number of observations.

From the firm and employee databases, we construct the following variables:

Year of foundation is computed as the minimum of the year of creation reported in the database, the year that the firm first appeared in the database and the year of hire of the first employee.

Month of foundation is computed as the month of hire of the first employee when the year of hire coincides with the year of foundation.

Survival S is an indicator variable equaling one for start-ups that survived their first two years. Firms are classified as non-survivors if they do not appear in the database in following years. In order to compute the survival rate, we exclude the last two years of the database (2007 and 2008). Firms can fail to appear in the database, even if they remain going concerns. For instance, a firm might fail to send the survey in by the due date for two consecutive years. Using data from previous years, however, we estimate that the probability of such non-response occurring in two consecutive years is less than one percent.

Size $Size$ is the start-up's initial number of employees. This measure is computed as the total number of individuals in the employee records in the foundation year.

Gender $Gender$ is a dummy variable equaling one for men and zero for women.

Age is coded in years in the database. We define four categorical variables: Age_{20-29} is coded one for individuals with age between 20 and 29; Age_{30-39} is coded one for individuals

with age between 30 and 39; *Age40 – 49* is coded one for individuals with age between 40 and 49; *Age50 – 60* is coded one for individuals with age between 50 and 60.

Education is measured with four categorical variables: *higheducation* is a dummy variable equaling one for founders with bachelors, masters or doctoral degrees; *mediumeducation* is a dummy variable equaling one for individuals reporting a high school diploma or vocational school degree; *loweducation* is a dummy variable equaling one for individuals that attended junior high school; and *veryloweducation* is a dummy variable equaling one for individuals who never attended or completed the elementary school.

Industry Experience *Ind* is coded one for entrepreneurs with experience in the same industry (four digit level) as that of the firms they found. Industry classification changed in 1994 and 2007, and there is no unequivocal relation between the old and new codes. To mitigate errors, we use all unique relations to translate old to new codes and, vice versa. Then, we compute the variable industry experience for the new and old codes and aggregate both results. Alternatively, we also use an algorithm, which is based on how the majority of firms changed industry codes from 1994 to 1995 to translate old into new codes. For 2007, this problem is mitigated because the database provides information on the new and old industry classification.

From the one-stop shop opening dates, we construct the following variables:

One-Stop Shop *ENH* (month) is a dummy variable equaling one for the month and county in which the one-stop shop opened, and for all the following months. Counties whose shops open in the first 15 days of a month will see the effects of the program in that month. If the shop opened in the last 15 days of a month, the effect of deregulation will begin in the following month.

One-Stop Shop *ENH* (year) is a dummy variable equaling one for the year and county in which the one-stop shop opened, and for all the following years. Counties whose shops open in the first six months of a year will see the effects of the program on that year. If the shop opened in the last six months of a year, the effect of deregulation will begin in the following year.